June 1, 1956

Dear Norton:

Your phone call yesterday must have been as upsetting to me as it was to you; I hope I am right in thinking it was made in the heat of the moment and that the air will have cleared before you visit Madison, which I am looking forward to very much. I was flabbergasted that you should have responded as you did to what I thought was merely a small courtesy, sending you that ms. before I had confided in anyone else about it, but I am trying to visualize the basis of the misunderstanding. Obviously you have been much more deeply committed to work on protoplasts than I had imagined: Rollin had told me simply that you had gotten some promising leads, but you couldn't really get at it seriously until the fall (and, what I couldn't understand, that your intentions must be keep a deep secret): I hoped the penicillin-sucrose approach would be of some use to you, and to anyone else interested in manifold problems of protoplasts. If I had known what you told me now, that you were really well along and deeply involved at the moment, I might have contacted you sooner, but should I have discussed the matter preferentially with you rather than anyone else working on protoplasts, e.g. Sol? In fact I am very uneasy about not having sent that notice to Sol, but as I promised to congeal the situation, I won't until your visit, or unless you are willing to release me from that promise. I had mentioned to Sol that I had stumbled into something like protoplasts in E. coli and would send him some specific details in a few days. Anyhow, Esther and I will be flying to Baltimore on Sunday June 17; we hope you can manage to spend at least a day with us, so least us know when and where to meet you.

Meanwhile, I hope you will weigh the merits of a more candid and casual approach. I know Rollin prefers to play his cards close to the chest, and of course that's his affair, but I hate to see you fall into the same trap. Just one unfortunate result of that strategy is precisely that it encourages an emotional involvement with intellectual "personal property". You have too many ideas and talents to have to be jealous of them, and in the long run secrecy doesn't work anyhow unless you propose to lock your lab door, estrange your friends, and enforce a moratorium on everyone else's imagination. We all have ego-centered motivations in our scientific work— you've heard my poser, how many people would endure research if all publication were strictly anonymous?— and we have to recognize them in order to cope with them, to use them as <u>constructive</u> drives. But if you don't think the facts of science are more important than the feelings of scientists, we have no common basis for this discussion. The most immoral suggestion I ever heard was that a fact of general interest should be suppressed, not to consolidate it, but for purely personal reasons.

Norton, I wouldn't want to 'hurt' you, and I just don't see how I have in any tangible way. Did you think I stole some of your ideas? is there anything about any mysterious rabbit tissue extract in the paper (of whose effectiveness neither you nor Rollin gave more than the vaguest hint)?—In fact, I was reading Dienes' paper in J. Bact this spring when it struck me that he needed hypertonic media for his L-forms, and these were enviously analogous to protoplasts; so when Rollin was here, I asked him whether he had ever happened to try penicillin + sucrose in combination. (I was speaking to his background on antibiotics, leaky bacteria, and penicillin-lysis). He didn't know, so I tried it on a Saturday afternoon with results that were rather startling to me (however they may compare with yours, which I know next to nothing about). This kind of an effect seems to have too general an interest to be sat on, on my view, so I spent a few weeks polishing up a quick survey just so you and everybody else could take advantage of it, for whatever use it may be in its manifold aspects. The paper I wrote on it is far from exemplary, but I'm not trying to prove anything, just put the technique on the market for public consumption. Naturally there are some aspects I would like to try to exploit further, but I have no hope of maintaining a fencearound any part of the garden.— Is there anything very original about using protoplasts? Or aren't we both aping Sol more than a little? I know I was tremendously stimulated by his account at Detroit; probably I would never have systematically looked for E. coli protoplasts, but an idea for tying them together with L-forms seemed worth following up a bit, especially if it can help explain the m/o of penicillin (which, by the way has caught the interest of some of Marv' Johnson's students; one was in this PM to show me some protoplasts he had made from a penicillin-sensitive B. subtilis.)

You have had every right to sit on your own eggs till you felt they were ready to hatch, and not knowing how ripe they were the penicillin ms. might have threatened to be an unintentional 'scoop'. This is easily rectified, and I would be delighted if you would consider writing up your own material now for publication prior to or cotemporal with my note, or any other way you want to handle it. I earnestly want to try to persuade you of this tactic, if you're ready, and urge you to bring a draft along such lines with you, whether you're entirely convinced yet or not. If you have any other proposal, I want to hear it, but I don't think I would be very sympathetic to suppressing the information about penicillin (and its too far along the grapevine by now anyhow). This seems like the most constructive course but I really don't know just what you have now, and you'll best judge that yourself. If you decide not to publish now what will you have lost by the time you are ready?

What I am going to write now is something of a lecture, but I couldn't help but be disappointed in what you said about Demerec over the phone. We both wish he would do a less messy job with parts of his work on transduction, but do you regret what he has added to it? He's "taken over" transduction by an energetic program which for all of its blunderbuss is turning up some remarkable contributions; he doesn't claim to have discovered the technique, and he is putting it to good use. None of this has depreciated your role in it, on the contrary it has focussed much more attention on transduction than it might otherwise have received. Instead of crabbing about it, do better yourself, as I know you can do if your work on that phage-host interaction

is as sound and beautiful as my impression of it. It would be very convenient for our ulcers if we had private gardens in science, but if you're going to work in a popular field, you have to cope with, in fact encourage, populab interest in it. If the intellectual exercise is your only motivation, and you can't stand the gaff, you'll have to work on something which is (by definition) 'less important'. I regret the tensions of competition as much as you do (and have had my share of nonsense and brilliance in it too), and we learn to accomodate one another to smoothe it down, but if you have what it takes, what can you lose? If it's integrity and caution that slow you down compared to the hotrod boys, you have to learn the price too, and comfort yourself with your own self-respect, but I hope in the long run that the same integrity is the only way to build up the longlasting esteem of your colleagues too. And I don't see what you've lost right now either.

I'm sorry you won't be at the Baltimore shindig, and that I'll miss you at CSH. I've been too busy myself to think of attending both (my main preoccupation when I'm not writing overlong letters to Norton being the incredibly complex job of assimilating the data for the 'trails' paper) or of contributing to either. There are some remarkable flubs in the choices of speakers at both meetings, particularly in the area of transduction (phage or DNA), but the proles need a break too.

My own work lately has been largely on the properties of different Hfrs; Larry is cleaning up the virology of lambda-transduction (burst sizes, especially, which interestingly enough comes out about one Gal+ per yielding cell, presumably the fragment of the original chromosome, while the Lp region only replicates qua phage); but worst of all, Kalckar has been enzymologizing the Gal mutants and claims that Gal_1, Gal_4 and Gal_7 lack the second step, UDGP transferase, while Gal_2 misses galactokinase. Shades of Demerec! We don't have the sequence, but 1,4 and 7 are in one position-effect group; 2 (and 8) are in another. (trans- heterogenotes within a group are galactose-negative).

Sincerely,

Joshua Lederberg